ADDRESS

Delivered by the President, Mr William H. Maw, on presenting the Gold Medal of the Society to Professor Ernest William Brown, February 8, 1907.

THE Gold Medal of the Royal Astronomical Society has this year been awarded to Professor Ernest William Brown for his "Researches in the Lunar Theory," and it is my duty to put before you on the present occasion the grounds for this award. In attempting this, I fear it will be impossible for me to do adequate justice to the importance of our medallist's work. This work is of so special a character, and its development has been marked by the introduction of so many original devices and methods of calculation, that an address such as this is quite unsuited for an examination of the details of the research. All I can hope to do is to put before you the broad features and general extent of Professor Brown's work, and to record the success which he has attained.

Professor Brown is the seventh astronomer to whom the Gold Medal of the Royal Astronomical Society has been awarded for work in connection with the Lunar theory. His predecessors were: Baron Damoiseau (1831) for his "Memoir on the Theory of the Moon," and for his Lunar Tables; M. Jean Plana (1840) for his work entitled "Théorie du Mouvement de la Lune"; Professor P. A. Hansen (1860) for his Lunar Tables; Professor J. C. Adams (1866) for his "Contributions to the Development of the Lunar Theory"; M. Delaunay (1870) for his "Théorie de la Lune"; and Dr G. W. Hill (1887) for his "Researches on the Lunar Theory." This is a long list of illustrious workers, and with them our present medallist is well qualified to rank.

In a paper entitled "Theory of the Motion of the Moon, containing a New Calculation of the Expressions for the Co-ordinates for the Moon in Terms of the Time," published in volume liii. of our *Memoirs*, our medallist has stated so clearly the nature of the problem on which he has been engaged that I may be permitted to quote from his introduction. He says:— "The formation of numerical expressions deduced as a consequence of the Newtonian laws of motion and gravitation which shall represent the position of the Moon at any time may be roughly divided into three stages. As a first step, we consider each of the three bodies—the Sun, the Earth, and the Moon—as a sphere of mass equal to its actual mass, and arranged in concentric layers of

equal density. The Earth (or the centre of mass of the Earth and Moon) is supposed to move round the Sun in a certain ideal elliptic orbit, and all disturbances of this orbit and of the Moon from any other source than the ideal Sun and Earth are neglected. This first stage constitutes nearly the whole of the labour of solving the problem of three bodies as far as the particular configuration of the Sun-Earth-Moon system is concerned. When this is done, we proceed to the second step, which involves the determination of the effects due to the difference between the actual and the ideal motions of the Earth and Sun, to the influence exercised by the other bodies of the solar system, and to the differences between the real and ideal arrangements of the masses of the bodies. The calculations so far may, theoretically at least, be made without any knowledge of the configuration of the system at any given time or times, beyond a general idea of the order of magnitude of certain of the constants involved. The third and final stage consists in a determination by observation of the various constants which have entered into the theory and the substitution of their values, so as to obtain numerical expressions for the co-ordinates in terms of the time."

As we shall see later, it is the completion of the first of these stages which has primarily been the object of Professor Brown's past labours; and as a result he has, after arduous work extending over the past fifteen years, completed the solution of the problem of three bodies for the case of the Sun-Earth-Moon with an accuracy very far in excess of that attained by any of his predecessors in this line of research.

Our knowledge of the motion of the Moon has accumulated during long ages; but it is, of course, only since the time of Newton, or, say, during the past two and a half centuries, that the Lunar theory has had any existence. Our earlier information as to irregularities in the Moon's movements was knowledge derived from observations, and it did not include any explanations of the causes to which these irregularities are due. Before the discovery of gravitation, all that could be done with the motion of the Moon was necessarily empirical. But even Newton, the discoverer of this principle, contented himself with the variation, the motions of the perigee and node, and the largest inequality of the latitude. Analytical expressions giving the position of the Moon in space were not seriously attempted until the middle of the eighteenth century, when three men simultaneously concerned themselves with the problem. They were Clairaut, D'Alembert, and Euler. The last-named, in 1753 and 1772, produced three Lunar theories nearly independent of each other, and of the third of these I shall have more to say later.

Of the six inequalities which affect the Moon's position by an amount capable of being discovered by naked-eye observations, viz. the effect of solar attraction in enlarging the Moon's orbit, the revolution of the line of apsides, the regression of the nodes, the evection, the variation, and the annual equation, only one—that to

which the name of "evection" was given by Boulliaud in the seventeenth century—was known to the ancients. It appears to have been first noticed by Hipparchus, about 150 B.C., when he was engaged in endeavouring to determine the Moon's distance, but it was first taken systematically into account by Ptolemy, to whom, indeed, its discovery has been attributed.

The variation, which has a fortnightly period and a zero value at syzygies and quadratures, does not affect the time of an eclipse, and thus escaped the notice of the Greek astronomers. It appears, however, to have been detected by an Arabian astronomer, Aboul Wefa, in the tenth century, but was lost sight of until rediscovered by Tycho Brahé about the end of the sixteenth century. To Tycho Brahé is also due the detection of the annual equation, although he gave it an erroneous value. A more correct value was determined by Horrocks, but its true character appears to have been first appreciated by Flamsteed.

Of smaller perturbations the number is almost endless—seventy such perturbations are, I understand, taken into account in the calculations of the Moon's longitude made for the American Ephemeris, and about half that number in the computations for latitude—but of these the most important, and in many ways the most interesting, is the secular acceleration of the Moon's mean motion; and on this I desire to say a few words. As early as 1693, Halley, after a consideration of the records of a number of ancient eclipses, arrived at the conclusion that these records could only be satisfied by assuming a progressive shortening of the Lunar month. A long period elapsed before this suspicion was confirmed, but in 1749 Dunthorne contributed to the Royal Society a paper discussing all available observations bearing on the subject, and the matter was further investigated by Mayer, Bouvard, and Burg. The explanation of the acceleration, however, long evaded the efforts of the mathematicians, but later the problem was taken up by Laplace, who at length, on November 19, 1787, announced to the Académie des Sciences his discovery that—"The secular equation of the Moon is due to the action of the Sun on the Satellite, combined with the secular variation of the eccentricity of the terrestrial orbit."

Laplace's first value for this acceleration was $11''\cdot135$ per century: a value subsequently reduced to $10''\cdot18$. The cause of this inequality acts on the Moon as gravity on a falling body, and its effect, therefore, is as the square of the time; but in carrying the calculations back to the time of the Chaldean observations, it was found necessary to add a small term depending on the time cubed. Thus if t = the number of centuries from the assumed epoch, the acceleration, according to Laplace, was equal to $10''\cdot1819\, t^2 + 0''\cdot01854\, t^3$.

These deductions of Laplace were verified in 1820 by two of our medallists, Damoiseau and Plana, and also by Carlini, their approximations being carried to a higher order than those of Laplace. The value deduced by Damoiseau was $10''\cdot72$, and by

Plana 10″·58. These values were all too large; but the error was at once fortunate and unfortunate. It was fortunate in so far that the values arrived at fairly satisfied the discrepancies between the records of ancient eclipses and the results of modern observations, and led to the acceptance of the explanation and the upholding of the gravitational theory; but unfortunate inasmuch as this close agreement had the effect of stopping for the time further research. Moreover, when some twenty years later Hansen took up the matter, he also arrived at a large value which he announced in 1842 as 11″·93, reduced in 1847 to 11″·47.

Things remained thus until another of our medallists, Professor Adams, took up the problem; and on June 16, 1853, in a paper read before the Royal Society, pointed out an important error in the work of Plana and Damoiseau, and showed that the correction of this error most materially reduced the value of the acceleration. Three years later, Plana was induced by Adams's investigations to re-examine a portion of his own work; and in April 1856 he admitted the imperfection of his theory, and deduced a result agreeing with that of Adams. A little later, however, he withdrew this admission, and deduced a value differing both from that of Adams and his own original result.

Adams's investigations led to strong discussions between the chief mathematicians of the time, and his deductions were not at once accepted. They were opposed by de Pontécoulant, and in 1857 Hansen published his *Tables de la Lune*, in which the value adopted for the secular acceleration was 12″·18. In 1859, however, the investigation of this part of the Lunar theory was taken up by Delaunay, another of our medallists. Adams had shown that in a certain series in m^2, m^4, etc. (the term m^3 being absent) the term m^4 had been wrongly calculated, and that as a result the numerical value of the secular acceleration must be approximately halved. Delaunay, carrying his calculations to m^4, obtained exactly the same result as Adams; a result which he announced in January 1859. This induced Adams to publish the value he had already obtained, using terms involving m^5, m^6, and m^7, the result being to give the coefficient of secular acceleration the value 5″·7, a result subsequently reduced to 5″·64. Again Delaunay took up the matter, and on April 25, 1859, he communicated to the Académie des Sciences the result of his investigations, confirming Adams's new term: and, by carrying the approximations to the 8th order, deducing the value 6″·11.

Into the further stages of this important controversy it is unnecessary to enter, but ultimately the results of Adams were accepted. The acceptance of his value, or of a close approximation to it, leaves the remaining discrepancy between theory and the results of observation to be explained by some other cause or causes; a work with which Mr Cowell is at present prominently identified.

The next important step to be noticed in the development of the Lunar theory is the epoch-marking work of Dr G. W. Hill.

The features of that work were most admirably dealt with in the Address delivered by Dr Glaisher on the occasion of the Gold Medal being awarded to Dr Hill in 1887, and it would be quite impossible to summarise them here. It need only be said that—founded to some extent on a suggestion contained in the paper (to which reference has been already made) published by Euler in 1772, namely, that of employing moving rectangular co-ordinates, but embodying entirely novel methods of development, of the highest interest from the point of view of both the pure mathematician and the astronomer—Hill's work opened out a new region for theoretical research, at the same time introducing great simplifications in methods of practical calculation.

Many other distinguished names will no doubt occur to you as associated with the development of the Lunar theory, such as Airy, Donkin, Cayley, and Newcomb, but time will not permit of my entering into the details of their work on the present occasion. I must pass on to consider some of the salient features which have marked the development of the Lunar theory; and in doing this I desire to express my indebtedness to Dr G. W. Hill for much valuable information relating to this branch of my subject, which he has most kindly placed at my disposal.

The incessant call for greater precision in dealing with the motions of the Moon has led to frequent repetitions of treatment of this subject, so that we are now in possession of ten or eleven Lunar theories, each professing to go over the whole ground. The advance in precision is obvious from the fact that while Euler contented himself with about thirty periodic terms in the longitude, Professor Brown has nearly four hundred. As would be expected, each investigator has adopted such a method as, in his judgment, would lead most promptly to the desired end. These judgments, however, from the necessity of the case, can be only probable conclusions. Thus the Lunar theories we have now before us exhibit much variety.

The broadest division of these theories which can be made is into the two classes of *literal* and *numerical*. For the first, all the quantities on which the result depends are represented by algebraic symbols; while in the second, the investigators have attempted to shorten their labour by introducing at the outset the numerical values of as many of the quantities as the nature of the problem admitted. The much greater labour involved in the elaboration of a *literal* Lunar theory has brought it about that of our ten or eleven treatments of this subject, only three are *literal*, viz. those of Plana, de Pontécoulant, and Delaunay. But this method of treatment is much more satisfactory to the mathematical mind, while it also possesses obvious advantages over the numerical method. In the earlier days of the treatment of our problem it seems to have been thought nearly impracticable to adopt the literal method, and to Plana must be accorded the merit of having first elaborated such a theory. Professor Brown's theory is partially literal and partially numerical. Of the five parameters involved in the pre-

sentation of the Lunar co-ordinates, four are left indeterminate, and it is only the ratio of the month to the year that receives a definite numerical value from the beginning.

We next have to note other varieties of treatment. In the earlier period it was a favourite method to adopt the true longitude of the Moon as the independent variable; that is, in the first instance, the mean longitude, latitude, and reciprocal of the radius were determined in terms of the true longitude, and there remained the task of inverting these formulas. This was the method of all the early elaborations, except the last theory of Euler, to which reference has already been made. On many grounds it could, no doubt, be defended; but it seems that it was Poisson who first threw discredit on it. His pupil, de Pontécoulant, adopted the time as the independent variable, and so also did Hansen.

I must now direct attention to the most prominent of the peculiarities which mark our medallist's elaboration of the problem. This may be stated as the complete utilisation of the circumstance that, setting aside the rates of motion of the elements of the arguments of the periodic terms, the analytical expressions of the three co-ordinates are capable of being separated into portions, each factored by certain powers and products of the four parameters which Professor Brown has left indeterminate in his formulas. These factors have been termed "characteristics." The advantage of considering these portions is, that each can be determined independently of the adjacent, so to speak, portions; while the treatment depends on the portions of lower degrees, which have been precedently treated. The credit of having introduced this notion must be given to Euler, who employed it in his last treatment of the problem, published in 1772.

Very closely connected with the foregoing principle is the device of making the Moon's mean longitude disappear from the equations employed in the treatment. Instead of using co-ordinates referred to fixed axes, there are employed those referred to two axes in the plane of the ecliptic having a velocity of rotation equal to the motion of the Moon's mean longitude. This contrivance is also due to Euler.

From the use of these two principles results a distinguishing mark in the treatment. Every treatment must begin with a stage called that of the first approximation, from which, by degrees, a superior precision is attained. All the Lunar theories mentioned, except the two particularly described, start from the Keplerian ellipse, or this modified by moving lines of apsides and nodes, as a first approximation. But in the last theory of Euler, and that of Professor Brown, the first approximation is the variational curve, already somewhat roughly derived by Newton in the *Principia*. It is the fashion now to call this a periodic solution, as the inequalities of motion go through all their varieties in a lunation; but we may arrive at a notion of the matter more simply in the following manner:—The motion of the Moon largely depends on the constant called the eccentricity, and this constant

might have the value zero; in like manner, the motion might take place in the ecliptic; the solar eccentricity might be zero; and, in fine, the Sun might be supposed at such a distance that its action in disturbing the relative position of the Moon might be regarded as always parallel to its action on the Earth. Let all these four possibilities be fulfilled; the result is a greatly simplified set of differential equations, easy of solution, although methods of approximation must still be employed.

Having obtained this solution, we are prepared to advance further. Let it be granted that the four mentioned constants, instead of having zero value, have values so small that their squares and products may be neglected. This supposition gives rise to a set of equations denominated by M. Poincaré as "equations to variation." They are of the class known as "linear," and are generally more easy to integrate than those called "non-linear." Their most important quality is their being, with the exception of the known parts, the same in all the stages of the approximation. It thus results that a considerable portion of the calculations made for the second stage of the approximations is still available for all following.

It may be stated that the series which satisfy the differential equations of a Lunar motion belong to the class now known as Lindstedt series, or those resulting from the addition of terms, each the product of two factors, one being a constant, the other a sine or a cosine of an angular argument, the latter being equivalent to the sum of positive or negative integral multiples of a definite number of elementary arguments. In the restricted form of the Lunar theory treated by Professor Brown the definite number is four.

In the elaboration of the integration it is not only necessary to determine the constant factors of the series, but the derivation of the motion of the elementary arguments. The latter have similar forms with the coefficients, and, like them, cannot be determined at one step; we must be content to derive the successive portions in their turn. In the Lunar theory the matter is somewhat alleviated by the circumstance that the motion of two of the four elementary arguments may be regarded as known at the outset; it is necessary only to derive the rates of motion of the mean anomaly, and the mean argument of the latitude. At particular stages it becomes necessary to notice that the previously-used motion of one or the other of the arguments needs correction by the addition of a new set of terms.

When the Lindstedt series are substituted in the equations to variation, the result is a group of linear algebraical equations in number precisely sufficient to determine the coefficients, or the corrections to coefficients, as well as the corrections to the motions of the arguments, if there be any. For solving the above-mentioned group of equations the most feasible method of treatment to adopt is still that of successive approximations. The most important quantity to be noticed in this solution is the determinant

of the equations. Not unfrequently this quantity is much smaller than the coefficients from which it is derived. When this is the case the coefficients involved must be computed to a correspondingly greater degree of exactitude. In two or three cases the matter is so pressing that three more decimals must be added to the values of the quantities involved. This is the most troublesome circumstance attending the elaboration of a Lunar theory, and variation of method does nothing towards the removal of it. It is very vexing for the investigator to find that he must return on his steps, and push certain quantities to a higher degree of precision. The matter is so complicated that it is impossible to prescribe *a priori* rules for procedure. Delaunay in his Lunar theory has noted, at the foot of the page, all the places where such a modification of process was necessary.

Such, in brief, is an outline of the method of elaborating a Lunar theory to-day. In his paper, to which reference has already been made, published in vol. liii. of our *Memoirs*, our medallist stated that he had then (1897) been engaged for six years in attempting to develop the ideas contained in Hill's "Researches in the Lunar Theory," by calculating the coefficients of terms with certain definite characteristics, to which I have already alluded; and he defines the "characteristic" of any part of a coefficient as being that part of its expression which consists of powers and products of the eccentricities, the inclination, and the ratio of the mean parallaxes. He goes on to say: "Dr Hill had obtained these which had the characteristic unity, that is, which were functions of the mean motions of the Sun and Moon only, and also that part of the motion of the perigee which was a function of the same quantities; Adams had done the same thing for the motion of the node. It remained, therefore, to obtain the general equations, to put them into forms suitable for calculation, and to show how the other parts of the motions of the perigee and node might be obtained."

I must now notice the degree of advance in our knowledge of the Lunar motions, attained through the labours of Professor Brown. When Professor Newcomb made his comparison of Hansen and Delaunay, we were in doubt as to the value of some of the coefficients in longitude to the extent of half a second of arc at least; also the motions of the perigee and node were uncertain to correspondingly larger quantities. On the other hand, the degree of accuracy aimed at by our medallist has been such that there should be included the coefficients of all periodic terms in longitude, latitude, and parallax which are greater than $0''\cdot01$ of arc, and that the results should be correct to this amount. That he has been able to carry out such a programme is assuredly a matter demanding our heartiest congratulations; in fact, with such a notable advance one is almost inclined to say that there is nothing left to be desired; however, this phrase has been so often upset in the past that it would be unsafe to employ it.

A natural question here arises: "What will be the effect of

Professor Brown's researches on the accuracy of Lunar tables based upon them?" This is a question of great interest and importance, but in the present state of our knowledge it is one to which it is difficult to give a definite answer. It has, however, been my privilege to receive an expression of opinion from Dr G. W. Hill, who speaks with the highest authority, and this opinion I may be permitted to quote. He says: "Much as we rightly welcome the results of Professor Brown's devoted labours, we should be unwarranted in assuming that their employment in the Lunar tables would give rise to a marked improvement in the representation of observations. A slight one indeed might be expected; but it has been evident for some time that the Moon deviated from its calculated orbit more because it is subject to irregular forces, which we have not yet the means of estimating, than because the tables are affected by slight defects in the mathematical treatment of the forces which are already recognised. This circumstance in no sense diminishes the credit due to Professor Brown's work." This is a very weighty expression of opinion, and it indicates that there is yet ample work to be done by investigators of Lunar motions. I should add that such research has been most materially aided by the important work of our medallist, who, by giving accurate values to the known perturbations, has defined more clearly the further irregularities of which the explanation has yet to be ascertained.

I have perhaps, in this address, dwelt at somewhat undue length on certain matters of a more or less historical character, but my excuse is that the facts I have stated may serve to emphasise the difficulty and the onerous nature of the work in which our medallist has been engaged. That he has succeeded in making great advances in a field of research which has received the deepest attention from the leading mathematicians of the world for the past century or more is in itself a most eloquent testimony to his powers. It must be borne in mind that in a problem of this character, the solution of which depends upon so many diverse terms, the investigator has not only to acquire such knowledge of the various terms as will enable him to discard those of which the influence is unimportant, but he has also, in the case of those retained, to devise such methods of treating them as will enable them to be practically dealt with in a reasonable period. In both these respects our medallist has achieved admirable results. Early in his work of calculating inequalities whose characteristics are the first, second, and third powers of the ratio of the mean parallaxes of the Sun and Moon, and the same powers of the eccentricity of the Moon, Brown found that the forms of equations then available left much to be desired, and were apt to lead to errors in the practical calculations; and in a paper entitled "Investigations in the Lunar Theory" contributed to the *American Journal of Mathematics*, vol. xvii., he showed how these difficulties could be avoided and the labour of computation diminished.

The thoroughly practical character of Brown's method of work * is most striking. Speaking of the value of Euler's suggestions, he has himself said:—"The working value of a method of treatment is not really tested by the closeness with which the first or second approximation will make the further approximations converge quickly to the desired degree of accuracy; the real test is, perhaps, the ease with which the final approximation can be obtained. Here we have the essential difference between the present method and all other methods. The approximations of the latter proceed along powers of the disturbing force. Euler's idea was to approximate along powers of the other small constants present. This gives a most rapid convergence, and a degree of certainty in knowing the limits of error of the final results which no other method approaches."

In a letter which I have received from Professor Brown he modestly states that the only portions of his work presenting real difficulties were those arising from the *direct* and *indirect* actions of the planets. On approaching these problems he found the subject in a somewhat chaotic state, and it was necessary to clear the ground and get the theory into good shape. In doing this, the first requirement was to be able to compute the derivatives of the Moon's co-ordinates with respect to n = the Moon's mean motion. But in our medallist's theory the numerical value of n had been substituted, so that the derivatives could not be calculated directly from it. Delaunay's literal theory might have been used for the purpose, but, owing to slow convergence, Professor Brown did not consider it accurate enough. Under these circumstances he succeeded in finding a method for getting these derivatives accurately from his theory, in spite of the fact that the numerical value of n had been substituted. The idea which led up to this

* The records of our medallist's work have been largely contained in papers contributed to our Society. In May 1897 Professor Brown sent in Part I., chapters 1 to 4, of the paper entitled "Theory of the Motion of the Moon, containing a New Calculation of the Expressions for the Co-ordinates of the Moon in Terms of the Time," from which I have already quoted; and this was followed, in February 1899, by Part II.; in May 1900, by Part III.; and in January 1905, by the conclusion, Part IV. [Parts I. and II. of Professor Brown's paper are contained in vol. liii., Part III. in vol. liv., and Part IV. in vol. lvii. of the *Memoirs* of the R.A.S.] Besides this, he contributed a number of papers which have appeared in our *Monthly Notices*, namely, "On the Mean Motions of Lunar Perigee and Node," and "On the Theoretical Values of the Secular Accelerations in the Lunar Theory," abstracts of which appeared in March 1897; a paper entitled "Note on the Mean Motions of Lunar Perigee and Node," published June 1897; one "On the Verification of the Newtonian Law," which appeared May 1903; two papers entitled respectively "On the Degree of Accuracy of the New Lunar Theory and on the Final Values of the Mean Motions of the Perigee and Node," and on "The Parallactic Inequality and the Solar Parallax," published April 1904; one "On the Completion of the Solution of the Main Problem in the New Lunar Theory," published December 1904; and one "On the Final Values of the Coefficients in the New Lunar Theory," contained in *Monthly Notices* for January 1905.

method was first stated in a paper * contributed to the Cambridge Philosophical Transactions in 1899; and the method itself, which is believed to be quite new, was dealt with in a paper † contributed to the American Mathematical Society in February 1903. Since then the calculations have been performed, but are not yet published.

The calculation of the *indirect* inequalities gave considerable trouble, but Professor Brown was able ultimately to show in a paper ‡ contributed to the American Mathematical Society in February 1905 that it was not necessary to calculate the perturbations of the Earth by the planets in order to get the resulting effect on the Moon, but that it was possible to go straight to the disturbing function of the Earth by the planet. The calculations of the *direct* inequalities were completed last autumn, and Professor Brown hopes to publish them during the ensuing summer.

The precautions taken by our medallist to secure accuracy in the final results have been most refined. In accordance with the original programme, every coefficient in longitude, latitude, and parallax, which is so great as one-hundredth of a second of arc, has been computed, and is regarded as accurate to at least this amount, the results being really obtained to one-thousandth of a second. To avoid the occurrence of errors of computation, equations of verification have been computed at every step of the work, every page of the manuscript having, on the average, not less than two test equations computed. I am indebted to Mr Cowell for the remark that our medallist is the first Lunar theorist to use independent equations of verification, thus creating a higher degree of confidence in his results than could ever come from mere duplicate calculation. It was his device to form the equation for a small variation of this solution of Hill's equations. Says Mr Cowell:—
"The numerical application of this device was rendered possible by calculating series for various complicated fractions of the coordinates in Hill's variation curve. The utility of the plan is obvious as soon as it is got into working order, and its conception implies rare insight on the part of our medallist. It lies at the root of his success in obtaining more accurate results, with less labour than his predecessors. He has also obtained theorems by which the higher parts of the motion of the perigee and the node may be calculated in advance of the corresponding group of periodic terms."

For the motions of the perigee and node, the final values obtained, and a comparison of these values with the results of

* "On the Solution of a Pair of Simultaneous Linear Differential Equations which occur in the Lunar Theory," *Cambridge Philosophical Transactions*, vol. xviii.

† "On the Formation of the Derivatives of the Lunar Co-ordinates with Respect to the Elements," *Transactions of the American Mathematical Society*, vol. iv.

‡ "On a General Method for Treating Transmitted Motions, and its Application to Indirect Perturbations," *Transactions of the American Mathematical Society*, vol. vi.

observations, were given in the paper published in the number of the *Monthly Notices* for April 1904,* to which reference has already been made. In giving these values our medallist pointed out that there was one constant of which the observed value was so far doubtful as to affect the results by as much as the tenth of a second, this constant being the ellipticity of the Earth. As there appeared to be two competing values for this constant, namely, $\frac{1}{292 \cdot 9}$ and $\frac{1}{296 \cdot 3}$, between which no definite choice could be made, Professor Brown decided to calculate the results for the two values. The final results, with the portions of which they are made up, are given in the subjoined table ;—

Final Mean Values of the Annual Mean Motions of the Perigee and Node.
Epoch 1850·0.

			Perigee.		Node.
Solar Action	Char^c. I.	+	148 524″·92	−	69 287″·90
	,, e^2	−	519·31	−	616·09
	,, γ^2	−	1 739·85	+	260·59
	,, e'^2	+	156·27	−	25·46
	,, α^2	+	2·24	−	1·11
	,, e^4	+	·04	+	·07
	,, $e^2\gamma^2$	+	6·72	−	1·70
	,, γ^4	−	1·51	+	·05
	,, $e^2 e'^2$	−	·99	−	·57
	,, $\gamma^2 e'^2$	−	1·61	+	·08
	,, rem^g.		·00 ± ·04		·00 ± ·02
Terms in No. 2		−	·70	+	·20
Terms in No. 3		+	·01	−	·01
Planetary direct		+	2·66 } ± ·06	−	1·42 } ± ·03
Planetary indirect		−	·20 }	+	·06 }
Figure of Earth (α)		+	6·57	−	6·15
Figure of Earth (β)		+	6·41	−	6·00
Calculated sum (α)		+	146 435·26 ± ·10	−	69 679·38 ± ·05
Calculated sum (β)		+	146 435·10 ± ·10	−	69 679·23 ± ·05
Observed		+	146 435·23	−	69 679·45
C − O (α)		+	0·04 ± ·10	+	0·08 ± ·05
C − O (β)		−	0·12 ± ·10	+	0·23 ± ·05

Note.—The "Figure of Earth" values "(α)" correspond to an ellipticity of $\frac{1}{292 \cdot 9}$, and those "(β)" to an ellipticity of $\frac{1}{296 \cdot 3}$.

* A slight correction to the values there given will be found in *Monthly Notices*, vol. lxv. p. 276. This correction has been made in the values as now given.

24

For the theoretical secular accelerations the values finally arrived at by our medallist (as given in his paper published in *Monthly Notices* for March 1897) are as follows:—

Theoretical Values of the Secular Acceleration per Century.

The Mean Motion	$+\ 5''{\cdot}91 \pm 0''{\cdot}02$
The Perigee	$-38{\cdot}9\ \pm 0{\cdot}1$
The Node	$+\ 6{\cdot}56 \pm 0{\cdot}02$

In devising the details of his research, our medallist arranged the work so that considerable proportions could be done by computers; but, as a matter of fact, only one—Mr Ira L. Sterner, of Haverford College, of whose ability and accuracy Professor Brown speaks in the highest terms—has been so employed. It may be interesting to quote here some details given by our medallist as to the time and labour expended on the work. He states: "From 1890 to 1895 certain classes of inequalities were calculated, but the work was only begun on a systematic plan, which involved a fresh computation of all inequalities previously found, at the beginning of 1895. Mr Sterner began work for me in the autumn of 1897, and finished it in the spring of 1904, though neither of us was by any means continuously engaged in calculation during that period. He spent on it—according to a carefully-kept record—nearly three thousand hours, and I estimate my share as some five or six thousand hours, so that the calculations have probably occupied altogether about eight or nine thousand hours. There were about 13,000 multiplications of series made, containing some 400,000 separate products; the whole of the work required the writing of between some four or five millions of digits and *plus* and *minus* signs."

Professor Brown has, as I have stated, completed his solution of the problem of three bodies for the case of the Sun-Earth-Moon, and has achieved an accuracy very far in excess of that of any of his predecessors; while he has done this by methods involving striking elegance and originality, and showing great powers of resource. He has, however, by no means finished his labours. As he himself pointed out, in announcing the completion of the main problem, much still remained to be done before it was advisable to proceed to the construction of tables. On this work our medallist is now engaged, and we may rest assured that he will continue to bring to bear upon it that energy and power of organised inquiry which have enabled him already to secure such brilliant results. We may, I think, further hope that in the present award he may find some encouragement in his labours.

I much regret that Professor Brown is not with us this evening to receive the medal personally, but a combination of circumstances—amongst them the serious illness of a relative—has rendered it impossible for him to cross the Atlantic at the present

time. This being so, I have been in communication with Sir Edward Grey, H.M. Secretary of State for Foreign Affairs, and I am glad to say that he has kindly arranged for the medal to be forwarded in the Foreign Office bag to His Majesty's Chargé d'Affaires at Washington, by whom it will in due course be transmitted to Professor Brown.

I will now ask Mr Lewis to receive the medal on Professor Brown's behalf, and to transmit it with our most sincere hope that he may long be spared in full health and strength to carry out the important researches to which he has devoted himself.

Before passing to other business, there is another matter connected with our medallist on which I should like to say a few words. I have not alluded to it in my address, because it has nothing to do with the award of the medal, but it will, I think, be of interest to the Fellows generally. As many present are aware, Professor Brown is an Englishman who has been long resident in America, and who has for the past sixteen years been connected with Haverford College. That association will, however, be broken in the ensuing summer, and next autumn Professor Brown proceeds to Yale University. It is exceedingly gratifying to know that his work on the Lunar theory, which he has been able to carry on at Haverford under most favourable conditions, will not be interrupted by this change. By a letter received from Professor Brown, I learn that not only have the Yale authorities recognised the importance of his work by arranging special facilities for its continuance, but they have also most generously undertaken to provide the funds required for both the preparation and the publication of the Lunar Tables which will form the natural outcome of our medallist's labours.